June 24, 1952

Dear Luca:

You give me undeserved credit for perspicacity: you had signed your last letter Luca.

Your explanation of the earlier result with Hfr x Hfr sounds very plausible (that one of the parents had become Nfr--F+?). I have casually tried to repeat the attenuation of F+, by simply keeping nutrient slants some months at room temperature, without result. Is it possible that your initial slant was a mixture of Hfr and F+, or had you reisolated the Hfr before noticing its attenuation. The shift from Hfr to F+ is evidently of some interest, and deserves to be verified. I don't recall whether I mentioned my concordant result with yours on Hfr x F- giving, exclusively, F-. The latter can be made F+ by transduction in the ordinary way. I am about to test the transmission of Hfr in haploid x diploid crosses, in which the Mal-elimination can be circumvented. It seems possible that Hfr is localized in the eliminated segment. I have no progress to report on Hfr cytology, largely because I have been occupied with completing a review (Cell genetics and hereditary symbiosis). This is completed, and I am back to the experiments again. I have not seen any distinctive appearances in mating cultures, and suspect that mating is a rather unobtrusive conjugation (as in Paramecium). I may have tioned evidences of cells stuck together, and of the blebs of some exudate joining cells in a rather suggestive way in Hfr + F-.

Our paper in Genetics should be rectified soon before it goes to the pr ter. Do you have any further comments? I would especially appreciate your explicit instructions on the form of your address (should "Serafino Balfan be included?). Also, are you obligated to mention financial support in a f note, as I have? I am looking forward to your account of the work fo JGM. I am told that the Genetics paper is rather stiff reading, but it des to be. I must ask that a foothote be included for acknowledgment of finan support, in the same form as you see in the Genetics paper. In the same pa graph, it can be mentioned that in the Genetics Dept. seriation, this woul be paper.No. 496. These formalities are a nuisance, but essential so that order reprints, and for other reasons.

I received a note from Dr. Penso a few days ago. I am sorry that I did realize its significance, and wrote back that I did not feel there was much be said about the genetics of actinomycetes. I would imagine that my remark would be more appropriate in the general section, or with the Enterobacteri (E. coli recombination and Salmonella transduction). I hope my reply to Dr. was not a faux pas! I would like very much for us to be able to visit you year, and greatly appreciate your efforts on my behalf, as well as your personal invitation.

Hayes has just written that 1) he has abandoned the identity of F+ with gametes, and 2) has some evidence of F+ transduction via filtrates. Mrs. Lederberg has some still doubtful indication of the same in lysates (which may have nothing whatever to do with the phage itself).

Your plan for presenting the JGM material is quite acceptable. I have little to add; as I mentioned, I would prefer not to comment on unselected crosses with Hfr until the story has been worked out. If anything else occurs to me, I will write promptly if anythi occurs to me. Do you have an address preferable to the Istitutp for the summer months?

I have no objection to "virus" for the F+ agent, but it may have confusing connotations of phage! In my review, I proposed the term Plasmid to

P.S. Esther finds WG-28 (NTCC 123 strains) self-incompatible; F+ can be effective ly transduced to it from K-12. In general, it behaves as F- in crosses with K-12 stocks, but with some inconsistent results that need to be clarified (x W-1607). Our first SRP survey, using W-1177 as tester, probably missed about half the total crossable strains, those which are effectively F-. The F system may not behave uniformly thorughout E. coli, however.

Do you know anything about NTCC 122? Weigle mentions this strain as sensitive to lambda, and with other peculiarities. Do you know anything about it? Is there some possibility he has 123? I am checking with him also, but experience shows that group may be careless about strain labels.

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cover all kinds of transmissible agents, whether plasmagenes, viruses, or symbionts, so long as they have hereditary functions. The only advantage of another new word is that it should have no connotations; in the present instahce, that may be very desirable.

I hope you have a more pleasant summer than we are experiencing. It is rather too hot and humid to do much work, but cytology is, for that reason, relatively attractive.

Yours sincerely,

Joshua

Joshua Lederberg